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Conversation with

Joshua Lederberg

Outstanding leaders convey their knowledge, skill, and personality to their organization—thereby stimulating growth and movement toward worthwhile goals. And often organizations bring out hidden talents in those who are chosen to lead them. In the case of Dr. Joshua Lederberg and The Rockefeller University, a high order of talent and an inspirational setting have been brought together in a rare combination.

If someone were to ask Dr. Lederberg whether he is a leader, his response would most likely be a prompt disclaimer. He sees himself not as a leader or a manager, but rather as a scientist surrounded by a community of outstanding colleagues who by working together are trying to lift the edge of the unknown—to their mutual exhilaration.

But Joshua Leaderberg is a

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manager—and a leader. And he sits in the center of a special place, The Rockefeller University. It has been only since July 1, 1978 that he has provided leadership to this world-renowned research and educational institution, but he has been a scientist and a spokesman for science all of his adult life. Never before, however, has he had 60 heads of laboratories, each an outstanding scientist exploring fundamental questions of medical science, "reporting" to him and looking to him for intellectual and administrative support. It is this very special quality of his role that makes his example interesting and potentially instructive.

Dr. Lederberg earned his Bachelor of Arts degree from Columbia University and his Ph.D. from Yale University. A pioneer in the field of bacterial genetics who received a Nobel Prize at the age of 33 for his investigations of the organization of genetic materials in the bacterial cell, he was a professor of genetics at the University of Wisconsin from 1947 to 1959. Earlier, as a research fellow at Yale University working with Edward Tatum, who in 1958 shared a Nobel Prize for showing that a specific gene controlled a specific protein's formation, he was the first to demonstrate the mechanism of genetic recombination in microorganisms. He left Wisconsin in 1959 to assume the chairmanship of the department of genetics at Stanford University, a position he held for 19 years.

Dr. Lederberg is active on numerous government and private advisory committees and boards dealing with such diverse fields as computer science, mental health and retardation, environmental health, biological-weapons disarmament, and space exploration. He is a member of the National Academy of Sciences and a charter member of its Institute of Medicine.

Even with this background, however, or perhaps because of it, his present position is a stimulating challenge for him. As he commented in his *Report of the President: 1978-1979*, ". . . I might describe myself as in a state of transitional reconstruction—after a leap from the laboratory bench to the boardroom—and wondering how to apply experience in scientific scholarship to organizational leadership. However, I have been confirmed in the belief that the inherent structure of The Rockefeller University lends itself to a president who has personally experienced the stresses, tedium, and thrills of the scientific pursuit. [What's] exciting about this environment is the responsibility that it places on the president to fit into a collegial framework, to be sufficiently informed or educable to enter into critical judgments about the wide diversity of research in progress, to help bring people together from different parts of the network, and to participate in the critical dialogue that is the substance of scientific progress. Most of the people at the university work in areas that are, in some measure, familiar to me and that I care deeply about. This heightens my sense of the unparalleled opportunity offered here for intellectual adventure and human service. Scientific research is one of the most enthralling pursuits that can occupy the human mind, and those of us who can dedicate our lifework to it are privileged indeed."

It is this attitude of being a participant in the process rather than a "manager" of events that seems best to represent Joshua Lederberg's perspective on his role. He sees himself as a colleague of the 60 heads of lab who participates fully in the intellectual excitement of discovery. To be sure, management and administrative tasks also fall to him—and those that he cannot delegate, he

performs. But it is the definition of the scientific mission of the university that is his main concern. It is preservation of the unique research identity of the institution that preoccupies him. It is the chartering of new fields for research that will capitalize on the special strengths already present in the university that excites him. He doesn't turn away from the tasks of administration and fund raising—critical responsibilities that are very much his—but he doesn't allow these tasks to divert him from providing long-term research leadership.

Leadership. That's the word. It is more than management because it means "Follow me." In a community of present and potential Nobel Prize winners, only a true colleague of such outstanding scientists could say "Follow me." But perhaps this is true in other organizations, too. Perhaps institutional leadership in general would be more effective if those who held the top position were better able to define perceptively the social mission of the institution and to move the institution toward its achievement.

(The interview was conducted by Ernest C. Miller, editor-in-chief, periodicals, for AMACOM.)

MILLER: What do you consider to be your most important present duty as president of The Rockefeller University?

LEDERBERG: To review the direction of the institution and develop strategies that will engage the effective energies of the faculty, students, sponsors, and other constituencies. Institutional identity is the first priority in strategic planning. Then we have to design the most effective structures, staff, and functional support to nurture creative energies and engender socially useful research. The organization of an overall consensus is facilitated here since our self-identification as a rather specialized biomedical research institute rests firmly on a renowned historical tradition.

A special quality of The Rockefeller since its founding in 1901 has been the breadth of basic biological and applied medical studies conducted here in a milieu that encourages the utmost intercommunication among the wide range of scientific perspectives and scholarly expertise. A result has been a record of accomplishment that identifies The Rockefeller as a major encampment of a continuing war against the diseases that afflict the human species. We take very seriously our motto "Pro Bono Humani Generis."

Some other related roles are economically and functionally compelling since they can be fulfilled at little incremental cost—for example, our educational program. With only 100 graduate students and 200 faculty members, graduate education obviously does not drive the institution, but we can hardly justify not having such a program, given the other resources in place.

MILLER: Does this concern about the objectives of the university reflect some change or challenge?

LEDERBERG:

It does, in some measure. Between 1901 and about 1955, there was no ambiguity about the nature of the institution: It was The Rockefeller Institute for Medical Research. During the mid-1950s, however, the board launched a substantial self-examination by the institute. A committee headed by Detlev Bronk, who at the time was president of Johns Hopkins University, was established. Serious consideration was even given to winding up the institute, I'm told, since it had already served as a prototype for the development of a medical research establishment in this country.

At that time, Dr. Bronk recommended greatly broadening the scope of the institution and transforming it into a graduate university that would still be generally oriented to a scientific mission. The thought seemed to be that it would evolve into an operational version of All Souls' College, Oxford, or of some of the graduate universities in Germany and that it would embrace not only the biomedical research specialties, but all the sciences—including the social sciences, and eventually the humanities.

This was proposed at a time of enormous expansion in American higher education and research in the wake of the role of technology in World War II. Dr. Bronk was sufficiently captured by the concept that he eventually agreed to leave his post at Johns Hopkins to become himself president of the institute in 1953. And he did set the institute in motion in that direction, expanded its facilities very substantially, pursued a much more aggressive policy with respect to investment in physical plant, and sought substantial funding from federal government sources. (Until that time the university had lived entirely on the income from its endowment.) While he was changing the style of the institute, he also set in motion changing its name from institute to university.

Under his leadership there was about a threefold increase in the scope of the university over a period of about a dozen years, but still the university did not broaden its activities to the extent contemplated. I have not seen a formal restatement of explicit goals during this period, but only a limited number of groups were established outside the medical sciences. There were programs in philosophy (since terminated) that were intended to be the nucleus of a humanities division, and programs in the behavioral sciences. A program in high energy physics and a program in mathematics and logic were also started.

Before this process of broadening the scope of the university's work could go much further—but after the institute had become a graduate university and had initiated a program of graduate education—it was more or less arrested by financial constraints associated with the changing national mood on funding science.

Starting in the mid-1960s, a substantial reexamination of those goals was undertaken. Dr. Bronk retired in 1968 and was succeeded by Frederick Seitz who, like Bronk, had been president of the

National Academy of Sciences. But while Dr. Bronk was a biophysicist and his predecessors had been Dr. Simon Flexner, a clinical scientist, and Dr. Herbert S. Gasser, a physiologist of some note, Seitz is a highly regarded solid state physicist and accomplished academic administrator. When he was appointed, the mainline specialty of the university was not, for the time, so sharply defined as biomedical research.

Almost as soon as Seitz arrived, the university entered into a period of severe financial stringency. His first major task was retrenchment and, almost certainly, a reexamination of what the university was about was associated with this process since a number of decisions about what programs needed to be sustained had to be made very quickly. His administration saw a gradual reversion toward the view that the historical strengths of the university had been in biomedical sciences.

When I was approached about the possibility of assuming the presidency of the university, I wrote a statement presenting what I thought an organization like The Rockefeller University ought to be, not knowing that a reconsideration of the university's mission was under way. I thought that my statement would probably resolve any problems or conflicts I might have by foreclosing an invitation to come. I was quite strong in my recommendation that the university should go back to its traditional strengths. In fact, I discovered that the trustees had been several years ahead of me in this respect—and so, indeed, had the faculty—and there was a very strong consensus that coincided exactly with my own views.

MILLER: How is The Rockefeller University organized to conduct its activities?

LEDERBERG: At a major university there are so many disciplines that are so diverse from one another that you must have compartmentalization. You really can't expect a student of medieval French literature to be able to

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discourse critically on the latest developments in molecular biology—or vice versa. At The Rockefeller University, however, there's a great deal of community of interest and comprehension of the subject and material of research by all the staff so we can and have avoided a departmentalized structure. That has been a major principle from the very beginning of the institute and then the university. The question has come up from time to time about what our internal structure should be, and typical departmental structures have been carefully avoided.

We have 60 laboratories and this is about the only organization the university has. The labs report directly to me.

MILLER: All 60?

LEDERBERG: That's correct. There's no hierarchical organization in between. We don't have separate schools, deans, or department heads.

Although the institution is a bit large to be able to handle this structure comfortably, it still works remarkably well. It works as well as or much better than any other structure I've encountered. We're about the same size as all but the very largest medical schools, for example. (We have an annual budget of a little over $40 million a year.) We are about the same size as Stanford Medical School or Cornell Medical College. At Stanford, there is a clinical division and a basic sciences division, and within those divisions there are separate departments—for example, of medicine, surgery, and OB-GYN, on the one hand, and of pharmacology, biochemistry, and genetics on the other hand. Then there are individual professors within the departments. And of course the medical school is but one of many operating units of the university.

We have none of that intermediate structure, and one consequence is that I do all my work in direct communication with the heads of labs, with the professors who are the main source of research initiatives. They are free to direct the programs within their laboratories. Furthermore, they have no structural barrier separating them from one another. I do have a support staff to help deal with matters like personnel, finance, and government relations.

We have a Friday afternoon colloquium, which is an all-university affair at which we have speakers, two times out of three from inside the institution. They speak to the entire institutional audience at a quite technical level, and it's a great success. It's one of the outstanding traditions of this place.

One of the questions that was asked of me when I was considering coming to the university, and then was urged on me again after I had gotten here, was please, could I make a real point of trying to attend those meetings! I have—and I wouldn't have done anything else. I have tried, on two occasions, to organize that kind of forum at Stanford Medical School, and it bombed out both times. There was

just not the same kind of interest in finding out what was going on in the other departments.

At Stanford the whole life of a professor revolves around the activities of the department rather than the university.

MILLER: How do you relate with these relatively autonomous heads of labs? I assume they come to you for funding?

LEDERBERG: Yes. We have an annual budget cycle and . . .

MILLER: Each lab has its own budget?

LEDERBERG: That's correct. The larger part of the manifest resources of a lab, however, is gotten from outside grants. About half the overall budget of the university comes from federal funds. Of the other half, a substantial part is already committed in terms of the salaries of the lab heads. The central discretionary funds that can be allocated differentially to existing laboratories are rarely more than about 20 to 25 percent of a laboratory's direct costs. The rest, the other half, goes directly to each of the laboratories in the form of research grants awarded by the federal government, lab by lab, on a project basis and, where they have a relationship with their scientific peers, discipline by discipline; this is obviously a fragmenting influence.

At a major (multirole) university the ratio of research support from outside sources would be even larger, so there would be even less of a motive for any kind of cohesive management of the overall program.

At most universities, the major managerial issue is the initial appointment of department heads. After that, the president or dean normally plays no role at all. There is a quality control process—a possible veto—with respect to appointments at a more junior level, but such appointments are decided entirely within the department. The original selection of an individual to fill a vacancy within a department is done entirely on a departmental basis. These appointments have to be reviewed by external groups, however, for quality control. I'm not complaining at all about the quality of those kinds of selections, but I'm suggesting that the overall process gets to be rather ingrown and that there is little cross-reference between departments.

One of the few occasions at Stanford when a basic scientist would have an opportunity of glimpsing what was going on operationally in a clinical department would be to sit on an *ad hoc* committee to review the appointment of, say, a professor of medicine or a professor of surgery.

In that kind of organization there is a focus on managerial skills for academic administration, and managing and doing are

separated. After a while, the dean is not expected to have any great insight into the actual texture of the work that is going on. He's expected to be a good manager, to be able to identify people who are capable of performing the tasks assigned to them, and to ensure that a workable organization structure is in place.

Within our arrangement, however, we have exactly the converse process, because we don't have highly formalized structures. There is much more of a burden and challenge on me and my staff to understand the scientific content of programs going on in different areas. Here we have a cadre of 60 potential slots, and when there's turnover in any one of them, that's a schoolwide issue. We have a zero-base examination in the replacement of any head of lab. It's not even known what subject will be involved because there are discontinuities when a professor retires. Such discontinuities cause problems in other areas, but they open up opportunities for innovation that really don't exist in highly structured organizations. In highly structured organizations the departments are the baronies: They determine the direction of the institution, and the institution becomes at best exactly the sum of its parts.

MILLER: Do you have any "jurisdictional" concerns among these 60 labs? Is it possible for two labs to be seeking grants in the same area without each other's knowledge?

LEDERBERG: That possibility doubtless exists. There are no formal mechanisms to override it. I view it as my job to be sure that the lab heads are informed about what the other labs are doing. But I am more concerned about helping to inspire insights that come from the intuitions of quite different fields, ideas that might never be thought about otherwise. But by and large the labs have a license to seek outside funding, the main limitation being what their scientific peers will allow them to have.

The main handle of central control is on space. And the allocation of space is a sufficiently contentious matter that there has been little effort to democratize that process. I think one can fairly say that I'm expected to know what the consensus is for resource allocation and be the voice of it, without requiring a formal procedure. I'll hear about it if the decisions I make don't make sense to a considerable number of people. But we do very little vote taking here; we have a much more informal procedure. They expect justice and fairhandedness on my part, and I hope I fulfill that expectation. If it sounds like a family kind of organization, you're right.

MILLER: As more and more funds have come from governmental and other external agencies, it would seem we've moved into a situation where we have an essentially demand-pull kind of research plan.

LEDERBERG: That's right.

MILLER: Is there any idea or "push" kind of research left?

LEDERBERG: Oh, it's possible to exaggerate on both sides. However, the project system of awarding funds does have very strong splintering tendencies. The survival of a laboratory depends on successful renewal of a grant application that has been delineated in one thick bundle of paper in a given year; you would have to go back three years later for renewal on that point. There are considerable disincentives to explorations in areas outside the mainline of one's original commitment.

The papers that you publish in the area in which you have already established a reputation reinforce that reputation and make it easier for you to obtain funds. In terms of funding, trying to get out of that rut can have quite harsh consequences.

I think there is a tendency to keep people in grooves they have already cut for themselves and to make it difficult for them to move into new areas and apply their imagination to totally novel problems—for them. There are no devils in this. It's just built into the way the grant process has evolved—with very tight competition.

Our own discretionary resources do not represent a large percentage of our total activity, but they can help us overcome the constraints of the grant system. My main responsibility in administering our discretionary funds is to allow some staff members flights of fancy—that is, to give at least some of our people some chance to do things they're just not able to do within the grant system.

I don't want to exaggerate the difficulties with the grant system. Under the system there are still opportunities to try out new ideas, as long as they're not a major part of one's time. If you are a very skillful investigator and can apply 60 percent of your energies to the central themes of your grant-funded research instead of 100 percent, and as a result have some time left to try other ideas, you can

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move out of your present groove a bit. But you don't advertise what you're doing, you don't make an issue of it until you've satisfied yourself that your research is going to be productive. Then perhaps you prepare a grant application that embodies some new direction and for which you already have enough evidence to make it a credible proposition.

The "pull" to a given research focus is also present, but not as much as some of the popular discussions of this would suggest. NIH, for example, is pushed in the direction of mission-oriented research by Congress, but I don't really see as much of that pressure from NIH as is talked about. It's undoubtedly true that there is some bias toward programs that promise early applications in certain fields, like cancer. At the moment, in fact, cancer research is better funded than is, say, mental health research. I don't think there are big differences in the levels of funding for basic versus mission-oriented research, however.

The Congress is not usually fully aware, of course, of how basic research has to be or of how long it takes to solve really hard problems. The disease of cancer is not something that can be dealt with overnight by any means. And targeting narrowly on it may be the least efficient way, in fact, to solve problems as hard as those involved. When we know what the problems are, we can find solutions by targeting work in that direction. But if we don't really understand the problems, research has to be much more fundamental.

MILLER: The difference between a NASA-type situation and an NIH-type of situation.

LEDERBERG: That's right.

MILLER: You commented earlier, when you were talking about your relationships with the heads of labs, that you keep well informed about their areas of investigation and make a point of communicating to other labs things you think it would be helpful for them to know.

LEDERBERG: Yes. You asked me how I do that.

At the colloquia I make a point of acting exactly as I did as a graduate student: I pop up with questions. I try to provide an example of not being afraid to appear ignorant or even foolish in asking questions. Sometimes my ignorance is feigned and sometimes it's real—I try to keep people guessing. The main point is the posture of not knowing and asking: an uninhibited search for knowledge.

I subscribe to ASCA service provided by the Institute for Scientific Information. This is a weekly alerting service that is profiled on The Rockefeller University, so I get a notice every week about every publication that appears anywhere in the scientific literature that had The Rockefeller University in the address. Typically, there

are 20 or 30 items a week. And some of my colleagues send me things spontaneously, sometimes more than I can manage. But I do try to read what they're writing.

We also have frequent sessions, which are part of our development effort, where we involve our faculty quite heavily in making presentations about the research going on in different laboratories. These are small colloquia at a lay level, but I attend almost all of these sessions. My colleagues are there, too.

We also have our annual scientific report, which gives a fairly good précis of what each laboratory is doing and of the publications of each lab's staff. I hope by the time the year is over to have gone through this in some detail.

I have made it a point, too, when I could, to visit our laboratories, and no one—yet—has hinted anything but pleasure at my coming and talking to them as a group. I've done that with perhaps 20 of the groups so far.

Last, I'm trying to revive the "Welch Hall tradition," which is probably impossible. This tradition is alluded to over and over again by almost everyone who has referred to the history of the institution, particularly its history during the time Simon Flexner was the director and there were only about 25 professors on the staff. The Welch Hall tradition involves a rather formal luncheon that was used for round-table discourse on specific scientific subjects, a sort of scientific luncheon club. There are many obstacles to trying to do that now. My colleagues tell me that the physical facility, which is now being used by the library, was a major factor in the success of the lunches. I try now to arrange luncheons of eight to ten of my colleagues, and I say to them, "Please, let's not discuss any administrative matters whatever, if you don't mind. Let's just talk together about the work that's going on in our labs."

I think that I'm accepted as a colleague and that I'm able to participate in a scientific discourse about work going on in a variety of fields. I wouldn't enjoy my job at all if it didn't require and give me the time to keep in touch with scientific developments. This is very important to me.

MILLER: Do you think the research leadership you provide is critical to your success as president of the university?

LEDERBERG: Well, I think by taking that role I can add a great deal to what the institution is capable of doing. I can't really answer whether it's critical in the sense of asking if the university could survive without it. I'm sure we could discover other ways of providing such leadership if I were not to do it, but it would make a difference.

I have to be very careful in how I participate in our research efforts. I don't try to prescribe. I don't want even to appear to be in that mode. The professor conducting the research is the specialist, but I can be a friendly critic. Criticism is so deep in the nature of the scien-

tific process that when it is offered with reasonable tact, respect, and compassion, it's the most valuable service one can provide one's colleagues.

I am developing a broad perspective about what's going on in the university so I can spot where there is relevant knowledge and insight in different labs that can and should be brought to bear on a given research problem. So I usually scribble four or five notes a day to my colleagues asking them "Are you aware of so and so?" "Are you onto this?" I usually preface my comment or question with "You probably already know this, but . . ."

None of this is formally necessary to maintain the organization. Our people have plenty of modes of communication without adding mine. But I think it does add an extra accent to the way we work, and I am trying to make it both effective and useful. Anyhow, it makes my job much more satisfying.

MILLER: To what extent are governmental and external relations and fund raising concerns that vest in you?

LEDERBERG: From the institutional standpoint, fund raising is absolutely crucial. It's perfectly obvious that no significant donor is going to invest in this university without having the opportunity to interview the head of the institution. The board wouldn't have considered me for this role if they had thought me incapable of articulating the purposes of the institution or of being able to translate these purposes to an intelligent lay public. I enjoy that kind of activity. I've had experience in establishing and maintaining those kinds of relationships, and obtaining such funds is very, very necessary. The flexibility of the university, our ability to determine our own directions and to explore different options, depends on private funding. There is no alternative.

I probably spend about a third of my time in fund raising, including public relations, mostly with private donors. We have to get $9 or $10 million a year from private sources, and we have to do it primarily from moderate to large gifts from a few individuals rather than from a mass appeal. The skills involved in trying to define a development program are rather special, and it's not the kind of thing that one can learn from a textbook. You need common sense, compassion for the interests and concerns of other people, and on-the-job experience. As president I am in a continuous process of justifying the university and of explaining what it is, why it exists, and why it's important. In talking with potential donors I must present the goals the university represents and why they can be pursued more efficiently by a philanthropic investment in this particular institution than by other means.

Our faculty deals directly with the government agencies providing funds; they're interested only in the pieces of paper that transmit grant applications, per se. And many foundation contacts for specific projects are made directly by faculty members, but some that

have broader scope and therefore involve more than one faculty member are handled by another member of my office.

MILLER: Do unrestricted corporate donations constitute a significant part of the private funds you obtain?

LEDERBERG: They are significant, and they may become more so. As time goes on, great personal fortunes will no longer exist. The only other obvious locus of both capability and responsibility for providing the funds that will be needed is the corporations.

MILLER: How do you think the effectiveness of the university is measured by its benefactors and the public?

LEDERBERG: Probably the only way it should be done is by the scientific reputation that the institution enjoys. You learn about its reputation in a variety of ways, but probably most effectively by just hearing how the university is regarded by the other contacts one has. I know the university is held in the highest esteem throughout the country, so I don't believe we have any problem in that respect. There are a few objective measures, too. You can count Nobel Prize awards and you can count memberships in the national academies and that sort of recognition. Even with our high standing, we feel somewhat uncomfortable with that sort of index because certainly one can point to large numbers of individuals who haven't received that kind of recognition but who are as highly qualified as those who have. But I don't think it's a coincidence that we have very high ratios on such measures. We have about 400 graduates, and I still find it almost unbelievable that two of them have *already* won Nobel Prizes. That's speaking for something.

MILLER: What is that?

LEDERBERG: Probably the appeal of the university for very, very qualified individuals, so we're transferring the locus of measurement of esteem. In

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effect, the kind of student who is going to win a Nobel Prize will decide to come here, and doubtless will have very good reasons for making that decision. At least he or she is not obstructed in his or her own development by the kind of institution we have. I would find it hard to say that out of a randomized set of students, the stamp of our education is what resulted in their getting such distinctions. I think our education is very good, but I think these students have been very well qualified in the first place.

MILLER: When you look for someone to join the staff, what are the major criteria you use in screening candidates?

LEDERBERG: Aggressive intellectual creativity and obvious skills in the particular area of interest are major factors. I also stress how a candidate would augment the community. The most brilliant individual in the world might not be suitable for our community, and I believe an individual's potential contribution to the intellectual community is a relevant factor, too. That's reflected in large measure in whether the individual brings a new dimension of scientific capability to his or her specialized field. There are also personality factors. We're talking about people who enjoy relating to others in this unique setting, where there is so much opportunity for interdisciplinary discourse; I feel that's one of the very strong assets of this institution. I would prefer people who will fit well in that environment, both to reinforce the tradition and to take advantage of it.

MILLER: The intellectual vigor and resources of the total community contribute to each of the elements of the community.

LEDERBERG: I think so. Now, you don't create that ensemble unless the units themselves have extraordinary capability as well.

MILLER: Is productive research still primarily a function of the outstanding individual, or is it primarily the team that is put together in a given lab?

LEDERBERG: With rare exceptions, it's impossible to conduct research these days without a fair amount of technical help. There is almost no laboratory where technical help doesn't play a significant role. I can think of one or two professors who have skills, literally, with their own hands and who therefore don't need such help, but they are the exception rather than the rule. The head of a lab designs the research strategies, selects the problems, develops the research techniques, and guides the work. He may only occasionally actually handle research materials with his own hands. But the groups may consist of from two people to forty. They vary depending on the field involved. Some areas of research require very complex coordination of different agents and instruments, and very specialized techniques; others can be handled by an individual.

We still don't have much of what we would call "big science" in the sense of large, complicated teams, usually with multifaceted direction, as you might have in physics or if you're working on a space program. If you're working on a million-dollar instrument, you don't do that yourself, at any level.

Our labs are such that, with very few exceptions, the professor at least occasionally actually handles the materials. If something is going wrong with a technique, the professor will come into the lab and work it through with his or her own hands until it's set right. The work will then be delegated to a technician or implemented by a fellow or student.

MILLER: Within the university community, do people move from group to group?

LEDERBERG: Not very much. Students do. We have graduate students, 20 to 25 a year. We have a small admissions office that guides that process. Our students are selected on an all-university basis; the students are not selected by individual laboratories. They quite often will involve themselves with several laboratories before they finally decide where they will do their dissertation.

The next grade would be our postdoctoral fellows. That's an informal level of training in the sense that we don't have a certificate or a named degree, but it is undoubtedly our most important educational output. These recent Ph.D.s usually stay for two or three years. Postdoctoral fellows are recruited directly by a lab, and their employment is generally funded directly out of grants that lab has received, although we do have some university training grants. As a result of how they're recruited and paid, they don't go from one lab to another.

We also have our assistant professors, a more senior grade of postdoctoral research worker. These are people we expect to be here for some years, although we could never offer them a high likelihood of tenure. They may be here for two, four, six, eight, ten years and have reasonably stable positions during that interval. But they might move around if a lab head were to retire, or if there would be some divergence of research interest.

We have a policy of requiring very careful review of the prospects of an assistant professor after no more than six years. That's done in this office. A lab head will make a recommendation, either for an initial appointment or for renewal, and will provide me with appropriate documentation about that person's performance. By the sixth year an assistant professor should either be a candidate for promotion to associate professor, which provides a further little notch of stability in salary and prestige, or should have been advised to leave, often because he has achieved maturity and should be in a more independent position than he has here. (There is a more hierarchal structure within the laboratories here than in other universities.) The issue is almost always resolved, however, by the fact that they reach

maturity and independence by the fourth year, sometimes the sixth year. The junior faculty members will already have been applying for research grants in their own names, and once they've got into that system and have been successful at it, they have a high degree of mobility.

MILLER: It's all very entrepreneurial. They go to the market, they are successful, and thereafter they can be independent in choosing where they will operate.

LEDERBERG: That's the scientific career these days! They learn how to work that system.

MILLER: How are associate professors and professors selected?

LEDERBERG: At the rank of associate professor and above there is a formal faculty procedure for reviewing those appointments similar to the *ad hoc* committees I described elsewhere. However, the initiative still comes from within the lab. It's only in the appointment of a full professor that we have a collective university process. We have a group called the Academic Senate, the group of heads of labs. Six or eight members of the Senate rotate by election onto the Academic Council. This committee is the guiding body with respect to academic qualifications when there's a nomination for an associate professor or the establishment of a Search Committee.

For a full professorship, there's an aggressive, no-holds-barred search that gives no preference to existing faculty. For associate professorships, candidates are nominated by a process that starts in the laboratory. I ask the head of the lab involved to provide whatever evidence he or she believes is most persuasive on the candidate's qualifications. I want to know that the person coming up to the rank of associate professor is of the quality of the best person you could hope to recruit on a nationwide basis.

MILLER: I'm not certain how the faculty at the university is rewarded.

LEDERBERG: We have annual salary review. That's done in this office. A component of this review is market driven because we feel we have to match what I know is going on in the rest of the country. And then some small margin is provided for the people we believe are performing their jobs with particular skill and insight and success; these are also the ones that other institutions would be likely to make a bid for. This is a matter that's left to the discretion of this office and the board of trustees; we don't have any public proceedings. What salaries people are being offered is regarded as highly confidential. I ask myself what is likely to happen if Harvard or Stanford makes a bid for so and so. Are we going to be so far behind in our financial rewards that he or she can't afford not to respond? But year after year—this is a national

phenomenon—our salary increases have been considerably less than the increase in the cost of living and as a result our staff members' real salaries are down 10 or 15 percent from what they were eight years or so ago.

MILLER: What is the nature of your involvement with your board of trustees?

LEDERBERG: It's fairly intermittent. The board, as a whole, meets only three times a year. The executive committee has occasional meetings at other intervals if some special problem comes up. Pat Haggerty [of Texas Instruments] is our chairman, and I have frequent conversations with him. He will often telephone other members of the executive committee, particularly to discuss policy matters. He is a strongly involved and experienced chairman. He does not involve himself in academic policy, but he takes on a very heavy responsibility with respect to the fiscal affairs of the institution, which is entirely appropriate. We are still in a deficit situation. We're okay in our operating budget, but our capital budget puts us below the line by about a half a million a year. But that's probably understating the problem, because it doesn't take account of the erosion of the real value of our endowment.

MILLER: Looking at the research scene on a national basis, do you think that the various institutional elements of our national research establishment are effectively coordinated and that we are making use of the comparative advantages of our different facilities?

Lederberg: My view is that to do more planning and coordination in Washington would make things worse, but that institutions should take more responsibility for coordinating their work with other similar institutions. I have been exemplifying this in what I have been saying about The Rockefeller University. I don't think national planning would work very well. I don't think anyone knows enough to be able to tie our national research effort together more effectively. The game is

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"The game is discovery. We're trying to find out what we don't know . . . and we cannot predict what might be important. The discovery of an important question is far more important than answers to what we think the questions are."

discovery. We're trying to find out what we don't know and don't understand, and we cannot predict what might be important. The discovery of an important question is far more important than that we get the answers to what we think the questions are. The rhetoric about centralized planning has gone far too far. Actions have not kept pace with the rhetoric, but matters have probably gone further than they should.

My main involvement with Washington is not in fund raising. I try to participate in thinking through the issues of science and research policy. A considerable amount of the time I spend in Washington is on such policy matters. I think there is a lot we need to learn about the process of stimulating worthwhile research. It's not a process that's well understood or that has been well investigated. How many people have the right to claim expertise on these matters? I have some observations about it, having been a front-line soldier. And the people who have had the same kind of experience and background as I had earlier in my career can be trained to ask some of the pertinent questions. But there's a great deal we don't know.

Discovery is the essential ingredient in scientific research, so we should seek to optimize the conditions under which discovery can be elicited. You can't possibly tell mature, experienced scientists how to make discoveries. Some of the things we do now, quite by inadvertence—I mentioned earlier the need for detailed specification of the research project and the dangers that are involved, and perceived, when you deviate from stated goals—make discovery difficult.

MILLER: Five years from now, if we were to sit here talking as we are this afternoon, what kinds of things would you like to be able to say about The Rockefeller University that you would be able to attribute to your being here?

LEDERBERG: I would like to be able to say, first of all, that we still exist proudly, with our traditions intact. That's no mean task. In fact, I would have to say that the university's tradition is such a strong one, such a productive one, that if that were all that could be said of my administration, it would still be very satisfactory.

I don't have aspirations for enormous change. We have a very special kind of place here at the university that is under a lot of pressure, and it takes a very responsible stewardship to keep it going. I think to do that will require all the measures that I have talked about concerning identity, mission, objectives, and so forth.

There are some subtle changes in style and texture that I would look forward to making and a very few programmatic efforts that I think will be important for us.

For example, with all the strengths we have in neurological research, at both a basic and a clinical level, in cell biology and in the behavioral sciences, we have no work at all now on psychiatric disease. Schizophrenia and depression are important public health

problems. With the perspectives that people already here will be able to offer to a neurological research program, we have enormous leverage to work in that area. I'm trying to start such a program.

Another programmatic effort that is a little less obvious but that has substantial social utility is an area I call comparative toxicology. Comparative toxicology involves providing a scientific basis for assessing the risk of toxic hazards from environmental sources. I think this issue is our most serious public health challenge today and at many different levels. First, our economy is now hostage to the accuracy of our perceptions about public risk, whether we're talking about nuclear power, the chemical industry, or the pharmaceutical industry. More and more the issue of the question of public liability comes up. In the past that has been thought of as a side effect that we can hope to clean up after the fact. I think that's a totally wrong conception. It is rather a basic question of the safety and adequacy of our procedures that has to become a central issue in the initial design of a technological innovation.

MILLER: In your mind, what is the focal issue in research management?

LEDERBERG: I've talked only in the vaguest terms about research management, although I think the job of management does come through clearly in what I've said. I think it's the job of management to define research goals but to be very cautious about centralizing control of the means, how these goals will be pursued. If you can convey your goals to the people who are actually confronting nature in the laboratory and get them to internalize them, that's about as far as you ought to go. They'll be far more capable of effective research when they are using their own imagination and direction and relying on the observations they make from day to day to guide their efforts to meet those goals than they would be in following the instructions of any central research manager.

The converse of that—and I don't think it is one that's sufficiently accepted as a research manager's responsibility—is that basic investigators ought to be left alone to make their own decisions about what research they will pursue. I think, however, that scientists have a responsibility to inform themselves about the needs of their immediate community—the social needs with respect to healthcare, for example. The biochemist engaged in basic research ought to know something about what happens in the clinic and what information is needed. I think if he's informed about that, he is not going to have too much trouble in developing an interface with the practitioner.